morphological speculations as simple mistakes. To compare an insect-embryo and its membranes with a Lamellibranch or an Ascidian in the extempore manner assumed so lightly by Mr. Lowne (p. 244) is not creditable. He tells us that he has no facts to guide him except the similarity of the form and disposition of the parts. Any reader who is not able to judge for himself should be very much on his guard when our author mentions Vertebrates or Ascidians, or indeed any other animals outside the class of Insects.

It is painful to speak with any disrespect of an author so laborious and so independent as Mr. Lowne. But these good qualities do not suffice to make a really good book. Advice will probably be thrown away, but we will offer one hint in the most friendly way. If Mr. Lowne before going to press would get his sheets revised by any cautious and well-informed zoologist, he would be saved from making statements which seriously impair his work.

L. C. M.

A Mendip Valley: its Inhabitants and Surroundings. By Theodore Compton. With Original Illustrations by Edward Theodore Compton. (London: Edward Stanford, 1891.)

THIS is an enlarged and revised edition of the well-known "Winscombe Sketches," and will be cordially welcomed by readers who can appreciate the presentation of natural facts in a poetic spirit. The author has spent the greater part of "thirty-three years of rural life" in the valley about which he writes, and every aspect of it he knows and loves. He tells much that is interesting, not only about the valley itself, but about the people who inhabit it, and about its archæological remains, its wild beasts, past and present, its birds, fish, reptiles, butterflies, and flowers. The style is simple and clear, and a certain charm is added to the writer's descriptions by the quaint reflections with which many of them are associated. An excellent chapter on the geological history of the Mendips is contributed by Prof. Lloyd Morgan. The illustrations are daintily conceived and executed, and harmonize well with the general tone of the text.

Key to Elementary Dynamics. By S. L. Loney, M.A. (Cambridge University Press, 1892.)

THOSE who are using the author's Elementary Treatise, whether they be teachers or students, will find this key very useful. The solutions to the examples are here worked out in full, so that even one who is going through the subject by himself will learn much in the nature of attacking problems by direct methods. The author's treatise is now so widely used that this key will come as a great boon to many.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Lightning Spectrum.

DURING the brilliant display of lightning on the evening of June 28, I took the opportunity of making some observations of the spectrum. The way in which the spectrum varied was very remarkable, some of the flashes giving apparently perfectly continuous spectra, while others gave a spectrum of bright lines, as already recorded by Kundt and others. The continuous spectrum appeared to be associated with the flashes of longest duration, which were accompanied by very little thunder, and the bright line spectrum with the more instantaneous flashes. Using a Liveing direct-vision spectroscope with a very accurate scale, I succeeded in measuring the positions of six lines in the green, all of which no doubt have been *observed* before, but in two cases at least the *positions* have not been previously measured. The wave-lengths of the lines observed were as follows—those determined by Vogel, Schuster, and Colonel John Herschel, being added for comparison:—

	Schuster.	Vogel.	Herschel.	Remarks.
(1) 5002	—	5002	5009	Brightest line
(2) 5168	5160	—	—	Rather dim
—	5182	5184	—	—
(3) 5350	5334	5341	—	Fairly bright
(4) 5430	—	—	—	Rather dim
(5) 5515	—	—	—	Fairly bright
—	5592	—	—	—
(6) 5675	—	—	—	Fairly bright

Other lines were seen both in the red and blue, but time did not permit any accurate determinations of their positions.

The lines (1) and (6) are undoubtedly the two brightest double lines of the air spectrum which occur in this region, but in the case of the other lines the coincidences are not so definite. The proximity of the line 5168 to the brightest carbon fluting (λ 5165) would suggest that it has its origin in the carbonic acid gas, which is always present in the atmosphere. The remaining lines do not appear to coincide with air lines, and their origins for the present are undetermined.

A. FOWLER.

Royal College of Science, South Kensington.

On the Line Spectra of the Elements.

PROF. RUNGE has not improved the position he has taken up by the new instance of a motion which he brings forward in last week's NATURE. The instance he gave in his preceding letter is a motion which, as I pointed out, could not take place within molecules. The motion he now gives is one which cannot even exist anywhere in nature. It would require a supply of power (energy per unit of time) increasing *ad infinitum*. The first instance he gave belongs to inapplicable kinematics, his new one to impossible dynamics. Neither has anything to do with the subject of my memoir.

He quotes the enunciation of a theorem from chapter iv. of my paper, but does not quote the sentence introducing that theorem, which would have made it plain that the motions spoken of in it are *motions which can take place within molecules and which can produce an undulation in the ether*, not the motions of a mere mathematical exercise irrespective of whether they are real or imaginary. The introductory sentence (p. 588) is in the following words:—"The motions of the electrons, the electric charges in the molecules, which are what excite the ethereal undulation, may be motions that are not confined to one plane. Accordingly *to study them* we must investigate what theorem corresponds to Fourier's theorem when the motion takes place along a line of double curvature." And then follows the demonstration and the enunciation quoted by Prof. Runge. In the foregoing words, in the introductory paragraphs of chapter iv. of my memoir, and in other passages scattered up and down through that chapter, I made it abundantly clear, as I thought, that I was dealing throughout *with a real physical problem of nature*, not engaging in mere mathematical exercises that travel into the infinite and impossible. I now see that I ought to have made more explicit statements upon this point for readers who would judge of each sentence apart from its context.

In order that a motion, $x = f(t)$, may be susceptible of treatment by Fourier's theorem, the following are conditions that must be fulfilled:—

1°. The motion must be recurrent, or capable of being approximated to by recurrent motions.

2°. The quantity represented by x must not become infinite.

3°. The quantity represented by t must not retreat.

I have been familiar with these limitations since I was a student, more than forty years ago. *They are known to all students.* I therefore thought it superfluous, and still think it ought to have been superfluous, to state them in my memoir. I thought it also irrelevant, *since none of the limitations could occur in the motions I was investigating*; and I wished to shorten my memoir by excluding all irrelevant matter. Prof. Runge, however, objects that I have not treated of violations of the first

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two of these conditions. He has not yet said that I ought also to have discussed the impossible dynamics in which the third condition would be violated.

This, however, was not his original position. He began (see his original article, NATURE, April 28, p. 607) by supposing that a motion from which light emanated cannot, if non-periodic, be investigated by Fourier's theorem; and he stated that in consequence of this he could not understand the decomposition of the motion of an electron within a molecule into a series of superposed elliptic motions. In NATURE for May 12, p. 29, and for June 9, p. 126, I demonstrated in two different ways that his supposition was a mistake. The other objection made in his original article, viz. that "a plausible suggestion about the movement of the molecules ought to explain more," is also a mistake. These are the two condemnations passed on my paper in his original article. Both these have been met. And the issues he has since raised are, I again submit, not *legitimate* criticism of a physical inquiry. To make them legitimate he would need to produce an instance of a motion *of the kind with which my paper deals* (*i.e.* a motion that can produce a spectrum) and which at the same time is not amenable to the method of analysis given in chapter iv. of my memoir. This he cannot do, for there are no such motions. In fact, the analysis effected by the spectroscope is *identical* with a part of that made by Fourier's theorem when applied in the way that I there point out. The spectroscope gives the periodic times in the different partials, the sum of the squares of their principal axes, and in some cases their forms; but it does not give the phases of the motions in them or the planes in which they lie. Prof. Runge almost admits that his criticisms do not succeed in impugning the value of my memoir as a contribution towards our knowledge of nature, for in his last letter he says, "I do not say, therefore, that Prof. Stoney's views on the cause of the line-spectra are wrong." This is very different from what he said in April.

G. JOHNSTONE STONEY.

9, Palmerston Park, Dublin,
July 17.

"The Grammar of Science."

MAY I, through your columns, point out to Prof. Pearson what seems to me a serious "antinomy," to use his own phrase, in his "Grammar of Science." The foundation of the whole book is the proposition that since we cannot directly apprehend anything but sense-impressions, therefore the things we commonly speak of as objective, or external to ourselves, and their variations, are nothing but groups of sense-impressions and sequences of such groups. But Prof. Pearson admits the existence of other consciousnesses than his own, not only by implication in addressing his book to them, but explicitly in many passages. He says (p. 59): "Another man's consciousness, however, can never, it is said, be directly perceived by sense-impression; I can only *infer* its existence from the apparent similarity of our nervous systems, from observing the same hesitation in his case as in my own between sense-impression and exertion, and from the similarity between his activities and my own."

With respect to the argument from the "similarity of our nervous systems," I may point out, *en passant*, that however many other people's nervous systems Prof. Pearson may have dissected, he has certainly never dissected his own, and that therefore this argument, which is several times repeated in the book, is worthless; all Prof. Pearson has to go upon is the external similarity of other people's bodies and activities to his own. But he maintains that our bodies and their activities are nothing but groups and sequences of sense-impressions. Consequently, if other consciousnesses are similar to his own, some of his groups of sense-impressions possess private consciousnesses, which themselves receive sense-impressions, among which, for example, are to be found Prof. Pearson himself! Thus Prof. Pearson's consciousness contains a number of parts, each of which contains, amongst other things, Prof. Pearson and his consciousness! Of course it would be impossible thus to refute a consistent idealist, who maintained that not only external things but all other consciousnesses were unreal and existed only in his imagination; but to recognize the reality of other consciousnesses is to recognize the reality of the means by which we become aware of them, which, as Prof. Pearson explicitly states, is the external aspect of men's bodies.

It is not difficult to find the way out of this difficulty. It is that, though we do not *know*, *i.e.* directly apprehend, anything about the external world but sense-impressions, yet in order to explain those impressions we frame the hypotheses of external, objective reality, and of the "ejective" reality of other consciousnesses, and since these hypotheses are successful in explaining most of our sense-impressions, we have come to believe that they are true. Indeed, I cannot seriously doubt that Prof. Pearson himself believes in them as much as anyone else. Only, if he were to acknowledge it explicitly, he would have to rewrite almost every page of "The Grammar of Science."

EDWARD T. DIXON.

12 Barkston Mansions, South Kensington,
July 14.

PROF. KARL PEARSON'S "Grammar" merits more justice than it has received from "C. G. K." It is a remarkable book which I have read with much interest. He tells us (p. 15) that "the unity of science consists alone in its method, not in its material," and therefore the method employed in this work on science acquires a special interest.

There are two points in respect to which his method seems to me to call for a few remarks—remarks which cannot be unwelcome, since his motto is "La critique est la vie de la science."

The first point concerns his own position and that of certain persons he freely criticizes. The Professor has scant patience with metaphysics, and says not a few hard things of those tiresome people the metaphysicians; and yet his own book is really a metaphysical treatise and he turns out himself to be an unconscious metaphysician *malgré lui*. This fact can hardly surprise anyone who has mastered what is really the scientific A B C; but in the present instance it is peculiarly amusing. For he, with great *naïveté*, ridicules Prof. Tait for being in the very same case. He is styled (p. 296) "the unconscious metaphysician, who groups sense-impressions and supposes them to flow as properties from something beyond the sphere of perception"; and we are further told that "the unconscious metaphysics of Prof. Tait occur on nearly every page of his treatment of the fundamental concepts of physical science."

The second point which seems to require notice is the way in which his method plays "fast and loose" both with the system he upholds and the system he most opposes.

He is an idealist of a kind. Again and again we are told that scientific laws are but descriptions of our feelings in conceptual shorthand. He speaks (p. 129) of "the whole of *ordered* nature" being "seen as the product of one mind—the only mind with which we are acquainted," and he tells us plainly (p. 130) that "the mind is absolutely confined within its nerve-exchange; beyond the walls of sense-impression it can logically infer nothing." It would be easy to multiply such quotations.

Now, of course the idealist can logically make use of ordinary language in describing co-existences and successions between his feelings. The Professor's distinctions (p. 114) between "physical and metaphysical supersensuousness" have been duly noted, as also his disclaimer (p. 57) of giving any real explanation of the physical side of thought. Nevertheless, none of these considerations appear to me to justify his dogmatic mode of speaking of things of which the senses can take no cognizance.

If he knows nothing but his own feelings, he cannot reasonably speak of their mode of formation, or of the manner in which one group of feelings acts upon another. Yet, referring to a sensory nerve, he writes (p. 51): "The *manner* [the italics are mine] in which this nerve conveys its message is, without doubt, physical," and (p. 81) "Beyond the brain terminals of the sensory nerves we cannot get." Stars are for him but "groups of feelings," and yet he writes about them as follows: "Among the myriad planetary systems we see on a clear night, there surely *must be* myriad planets which have reached our own stage of development, and teem, or have teemed, with human life" (p. 179).

Speaking of waves (p. 305) he tells us, "The wave *forms* for us a group of sense-impressions." But the wave is, for him, *itself* a group of self-impressions and so is a particle of protoplasm. Nevertheless he speaks (p. 413) of the probability that long stages of development preceded its existence, and "of the millions of years, with complex and varying conditions of temperature," needed in order "to pass from the chemical *substance* of life to that complex *structure* which may have been the first

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